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ABSTRACT

This paper discusses four kinds of reasons for studying child language. The first of the four, biological reasons, includes the desire to understand our own species and its place among other living things in the universe. The common human faculty for communication, the variability in language building, and the similarity of human communication to that of other species' are explored. The second group of reasons is social and addresses two qualities that distinguish human from other species' social behavior: (1) the existence of cultural differences; and (2) the continual construction and reconstruction of social reality by communicative interaction. The third set of reasons for studying child language is cognitive and acknowledges that knowing and thinking are states or activities highly characteristic of the human species and are worth systematic investigation. The fourth group of reasons is interventionist and focuses on normal language development as a basis for education, remediation, or therapy. It is proposed that by studying the playful, structure-building plasticity of child language development, we can answer the biological, social, cognitive, and interventionist inquiry. (MSE)

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Why we study child language

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It is a happy occasion when one can spend an hour explaining and justifying one's professional interests to a captive audience. I first became interested in the study of child language development as a graduate student in the 1940s when I took a course in the psychology of speech and also had the good luck to read Jakobson's Kindersprache, which had just appeared. But I wasn't able to devote any time to research or teaching on child language until twenty years later. From the 60s to the present I have been able from time to time to devote my attention to child language as one of my professional interests. In my remarks this evening I have no intention of trying to analyze how the events of my own life history might have led me to study language development or what personal goals or motivations might have been involved. The question I would like to address is the more general professional question of what legitimate intellectual issues justify the systematic study of child language, a topic at first glance far removed from serious academic disciplines in the arts and sciences.

There are, of course, many reasons for studying how children learn to understand the talk of people around them, to produce their own talk, and in many societies to read and write as well. I'd like to talk about four sets of reasons that have seemed important to me.

The first of these are, I suppose, biological reasons. We want to understand our own species and where it belongs among other living beings and in the universe as a whole. The most obvious place to start is in finding out how our body works, and this means observing the changing forms and functions of the body from conception to death, what could be called developmental biology. In recent years, to be sure, developmental biologists have become so justifiably fascinated by genetics and molecular biology that they hardly ever get past birth in their studies, let alone the maturation through childhood to adulthood. Yet in our species, as in many others, the development between birth and adulthood is not only as complex and mysterious as the first cell divisions and fetal development but is even more distinctive of the species in view of its neoteny and, of course, it is closer to the adult form and function and hence presumably easier for us to grasp, at least in a preliminary way.

One of the most salient characteristics of our species is the complex system of intraspecific communication that it exhibits. Other animal species, and even plants, have their own communication systems, but the human system seems by orders of magnitude more complex, more pervasive, more central to the existence and flourishing of the species. Just as we have a digestive system, a circulatory system, a reproductive system, and so on, it makes biological sense to say we have a communicative system. The fact that most of the organs involved in the perception, internal processing and

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production of language are also used for other functions does not exclude the complexly coordinated "system" nature of communication. Every normal human child, given a certain minimal exposure to language in use, acquires an incredible array of fantastically coordinated behaviors of language and related communicative activity. The obvious and almost completely unanswered questions are: How do we get from genes to adult language behavior? How did our species get from no language to language? The most obvious place to start to answer these questions, in spite of undoubted pitfalls and deadend byroads, is the study of child language.

Let us take an amateur ethologists's approach. Can we find some insightful analog to the development of the complex set of communicative behaviors? My favorite is one that Bickerton has often used, the nest building of various species of birds. At the appropriate point in the life cycle, birds of a given species engage in a set of behaviors of gathering materials, taking them to a selected place, and constructing a nest of a certain size, shape, and form of construction, and depending on the species, going through this routine only once, or annually, or several times a year. The point of the analogy is that the birds seem to know how to do this. Each species has a typical range of preferred materials, locations, sizes, shapes, and forms of construction, but of course the exact materials, etc. will be in part determined by what is available. It is certainly tempting to think of human language in these terms. Members of our species have in their genes (combined of course with the necessary interaction with the environment, including fellow members of the species), a fundamental plan for language-building. The exact details will, of course, depend on the particular materials available. If the materials available in the environment are the sounds and meanings of Chinese, the resultant language that is built will be Chinese-shaped; if the materials are the sounds and meanings of French, the resultant language will be French. But the basic plan of language-building will be the same. That is, there must be some set of genes the interaction and development of which specify this basic plan and its adaptability to local materials.

It takes only a few moments of consideration to find serious flaws in this analogy. Not simply that human language behavior is more complex than avian nest building, nor that it is proving extraordinarily difficult to discern the universal plan underlying both Chinese and French. These two differences -- the complexity of language and the difficulty of specifying Universal Grammar -- could be acknowledged and the insightfulness of the analogy still pursued. What for me destroys the usefulness of the analogy is the existence of multiple language building. A human child raised in a French-Chinese bilingual environment will build both nests/languages simultaneously. The genetic endowment of the child is such that he or she is able to discover that the input contains two sets of materials that are to be treated selectively and built in two different ways.

This capability of the human "faculty of language" cannot be overemphasized. Human beings can acquire different languages simultaneously or successively. A second or nth language may be acquired in childhood or

old age; from spoken, written, signed, or other input. A child raised in an environment with both English and American Sign Language will be bilingual in those. But child bilingualism is not the main point of this variability in language building. A little closer to the main point are the phenomena of child bidialectalism. Children raised in an environment where two dialects of the same language are spoken may acquire only one of the varieties, or both (i.e. become bidialectal), or may create a compromise variety different from either. And this whole picture must be suitably multiplied by numbers of possible dialects or languages. Humans are incorrigibly language building, even though in our dominantly monolingual American academic community we may lose sight of this.

The main point of variability in language building is actually slightly different from the bilingual/bidialectal issues. Human adult communicators vary their signals depending on the identity of the addressees; the time, place, and purposes of the communication; the content of the communication; and a host of other variables. Put more generally, language users show "register" variation, i.e. variation depending on occasions of use. In our own speech community, for example, we know that sports announcers' talk on the TV, mothers' talk to young children, teenagers' phone conversation, and classroom lectures are different kinds of English. People on different occasions may have different meanings to communicate, but even when they have essentially the same meaning to communicate they do so with different lexicon, phonology, syntax, forms of discourse, and, of course, means of non-verbal communication. Every normal adult human being has an extraordinary range of register variation, and some individuals have fantastic special competences in this regard. More important for our consideration, all children demonstrate register variation even before they produce their first recognizable words of the ambient language. Very early, children learn to communicate differently with familiar caregiver versus adult stranger, with mother angry versus mother playful, and so on. As Andersen (1984) and others have shown, four-year-olds can be very skillful at register variation. They not only have built different ways of talking to younger siblings than to peers, and so on, but they are able -- when suitable elicitation techniques are used -- to demonstrate how they think fathers talk to mothers, patients talk to nurses, and teachers talk to pupils, even though their own experience with these occasions of use has been quite limited.

Other animals show some differences in their signaling depending on the occasion -- watch a community of gibbons interacting -- so in principle this is not a surprising characteristic of human language. But the complexity and subtlety of this variation is a fundamental property of human language, perhaps more important than many of the other so-called "design features" that have been listed. It seems likely to me that this capacity to vary one's language registrally is fundamental to many of the other surprising communicative capabilities of humans. In a bilingual community where one language is largely ergative and the other essentially nominative-accusative in structure, children's ability to sort them out and acquire both is more deeply anchored in their capacity to adjust communication to context than it

is in some kind of grammar evaluating or parameter setting, important though such or similar processes may be from other perspectives.

If we ask ourselves what is likely to be found in that 1% or so of genes by which the human species is said to differ from chimpanzees that would make human language possible, my suspicion is that an expansion and reorchestration of communicative adaptability would be there. Human beings pay great attention to the cooccurrences of signals and contexts. The extreme situatedness of human communication is one of the big factors to explore. And the study of the growth and implementation of this capacity in the child's language would well repay the investment in time, effort, and research ingenuity that would be required.

I cannot leave the realm of biology without recalling an encounter I had some years ago in jointly teaching a course on Language and Mind with another linguist. It so happens that I was raised in a religious tradition and have myself a Christian commitment, but in that course I found that my curiosity was about how the human species came to have language and what aspects of human language were shared with other species, an evolutonary perspective. My colleague was raised in a secular tradition, and had, as far as I knew, no particular religious commitment, but his curiosity was about the shape of the universal language plan, not about any possible connection with other species, a clearly creationist perspective. The irony of the contrast was not lost on either of us, but we continued in our respective curiosities.

Well, so much for biology.

The second set of reasons for studying child language might be called "social". The human species like many otbhers, ranging from the so-called social insects to various mammals, is highly social, in the sense that there is extensive role specialization depending on one's place in social groupings, and many behaviors are explainable in terms of contributions to the community rather than for one's self and immediate family. In two ways, however, the human species seems qualitatively different from others in social behavior -- the existence of cultural differences and the pervasiveness of the continual construction and reconstruction of social reality by communicative interaction.

Every social group is characterized by a set of beliefs, values, material objects, and practices of all kinds that are learned and shared by members of the group and transmitted from one generation to the next. Each culture, that is, each particular structured aggregate of such features, is remarkably persistent over time even though continually changing in details and even in aspects of its overall configuration. Although some marginal instances of cultural differences between social groups can be found in other species, this business of having extensive behavioral differences between populations of the same species is one of the outstanding characteristics of human beings and hence something to be investigated and understood.

One of my favorite examples of the persistence of culture over time is the history of interaction between German missionaries in Australia and aboriginal groups they chose to preach their message to (cf. Fergson 1987). The best known story is that of the mission to the Aranda in central Australia begun in 1877.

The German missionaries took for granted not only the unique validity of their Christian doctrine, but also the universality of a set of valued practices, such as:

1. fixed times and places for eating, sleeping, worshiping, working;
2. personal cleanliness and elaborate clothing;
3. patrilineal family names and a simple kinship system with few constraints on marriage partners;
4. raising food crops, keeping domestic animals, and working for payment;
5. ownership of real and personal property, saving resources for future contingencies;
6. speaking a superposed standard German in addition to local dialect;
7. pervasive role of mother-tongue literacy: books, letters, records, lists, diaries;
8. pervasive role of numeracy: counting, measuring, comparing, doing arithmetic;
9. formal schooling for boys and girls, including studying foreign languages.

The Aranda took for granted traditional beliefs in an ancient dreamtime when their superhuman ancestors roamed the land, and also such cultural features as:

1. nomadic existence, moving in small groups from place to place in search of food, building temporary shelters or none at all;
2. no notion of personal cleanliness, and little or no clothing;
3. no family names but a very complex system of generational kinship classes that severely constrained possible marriages and prescribed patterns of social avoidance plus totemic clans that identified "ownership" of ceremonies tied to natural features of the land;
4. hunting and gathering, with fantastic abilities in tracking and hunting (a young boy might be able to identify the footprints of each of 200 persons meeting for a ceremony);
5. very limited real property, land ownership primarily in terms of the right to control appropriate ceremonies, no saving, eating food immediately when found but elaborate obligations for sharing food items with various relatives;
6. interaction across language groups, people picking up a different patrilect or language as needed;
7. no notion of literacy;
8. no notion of numeracy: Aranda has number words for one, two, three, and no higher; expressions for "more" and "less" rarely used; no notion of measurement; (yet able to quickly identify playing cards by non-numerical

classification, and playing children's game of star "counting" by matching configurations, without numbers);
9. formal teaching only with regard to details of kinship terminology and the traditional mythology and ceremonial lore in preparation for secret initiation rites of boys; only documented example of intellectual pursuits the discussion of the kinship system. ("...considerable enjoyment and satisfaction was derived from erudite discussions of the ramifications of the system, to which a great deal of time was devoted. One cannot but conclude that the pleasure was an intellectual one and that the activity was analogous to those indulged in by academically minded people the world over." Seagrim & Lendon 1980:34.)

Even from this brief summary the extreme cultural clash is quite evident. By the 1950s and 60s, however, the two groups had made considerable adjustment to each other and to mainstream Australia. The German missionaries acquired English as a second language, often speaking more bookishly than the English-speaking Australians, and subsequent generations moved from bilingualism to English dominance and finally English monolingualism. The Aranda began to acquire English and by the 1960s most of them knew some English, many of them speaking a somewhat pidgin-sounding non-standard variety, but some of them fluent and even English dominant. Most of the Aranda had family names and Christian given names. The Aranda had become staunchly Christian and there were several ordained Aboriginal pastors. Hundreds of Aranda were settled around the mission center of Hermannsburg. Aranda hymn singing was enthusastic, Aranda children were attending school, Aranda adults had learned to wear clothes and buy and sell.

But in the 1970s an astounding series of events took place. As the mission administrators moved first to encourage the formation of an Aboriginal council and Aranda policy making and finally to turn the title of the mission reserve to the Aranda, they were surprised to find that the Aranda did not recognize the authority of the council they had themselves elected, and instead of accepting overall Aranda ownership of the land, small groups began to "secede" as it were and move out to traditional ceremonial sites. It turned out that in spite of the apparently thoroughgoing changes in Aranda behavior, the old kin names and totemic identities had been maintained, knowledge of traditional places "owned" was largely intact, and many boys had been initiated secretly to avoid offending missionary and church leaders. The majority of the Aranda settled in Hermannsburg reverted to a life style much closer to the traditional. The new social groups were all eager for certain kinds of schooling and arranged to have one teacher assigned to each group, giving instruction outdoors in a way that was hard for Australian government authorities to fit into established patterns of education.

We might add here that it remains true that many Aranda children are good trackers with impressive observational skills but poor at literacy and numeracy even after years of schooling. Children with the least European type of experience were often Piagetian non-conservers at age 9 and 10; in

fact some adult Aranda do not believe that water poured from one container to another of different shape is necessarily the same amount.

The mystery is evident. How do social groups succeed in conventionalizing their particular cultural configuration? How do they manage to transmit the pattern to succeeding generations? How do they manage to change the pattern in response to external events and to internal forces for change in their own group? These are all the same question: How does a culture become conventionalized, i.e. shared by a group?

Language is at the same time a prime example of socially conventionalized behavior and one of the principal means of learning, sharing, and transmitting all of culture. Consequently one of the most obvious places to start in the attempt to fathom the mystery of conventionalization is with language behavior. For example, how do the members of a speech community come to share the thousands and thousands of details that constitute their mental lexicons? A neurologist has been quoted as saying "I must confess that I have always been more impressed with the capacity of the human brain to discriminate, characterize, and store in memory the thirty thousand plus arbitrary words in active use than with the complexity claimed to be involved in learning a few dozen syntactic algorithmic rules" (Marin 1982 as quoted in Aitchison 1987:202). The neurologist may well be impressed by the individual's astonishing achievement here, but I am even more impressed by the fact that members of the community share to an incredible degree this mass of detail. And, of course, even if we did put the lexicon at the center of language there would still remain the countless details of syntax, phonology, forms of discourse,and all the rest of language behavior that are shared. Some substantial part of this social competence may be in some sense universal and built in, but any speaker of English learning Aranda or any speaker of Aranda learning English or German will be forced to recognize an enormous mass of detail that distinguishes one language fom another, a mass of detail that is socially shared in the target speech community.

There are at least three places where the study of language conventionalization seems very promising: the study of language change in progress, the study of second language acquisition, and the study of language development in the child. In recent years, beginning with Labov's (1963) study of a sound change on Martha's Vineyard, we have seen more and more sophisticated study of language change in progress, and the workshop tomorrow afternoon will offer us a number of perspectives on this kind of research. The study of second language acquisition is just beginning to achieve the kind of sophistication needed for progress in understanding this kind of conventionalization, and recent shifts in grammatical theory and new approaches to discourse analysis are stimulating SLA research (cf. Flynn & O'Neil 1988 and Ferguson & Huebner, forthcoming). But of the three possibilities I find the study of first language acquisition the most promising. Of course, since at least in our society, and some other similar ones a crucial stage is when the child shifts from attending to the models of early caregivers to the models of child peers, we need careful

longitudinal research that combines more traditional psycholinguistic study with the sociolinguistic research methods of such linguists as the Milroys, Payne, Lodge, and Eckert.

Another comment here. Some outstanding developmentalists have conducted extensive cross-sultural studies of socialization, the most famous probably being the Whitings. But sadly, from my perspective, they paid no attention to language as such. Fortunately, a few child language researchers are attempting to repair this lack, and we have had Schiefflin, Ochs, and Heath reporting on their research at this Forum in other years.

In a fuller version of this talk I could deal further with the interactional construction of language conventionalization and change, but let me now just move on to a third motivation for child language study.

The third set of reasons for studying child language are cognitive. Each individual human being "knows" and "thinks" and "reasons". Certainly among the most basic questions we can ask about ourselves are: What does it mean to say someone knows something? How do people come to know things they previously didn't know? What is happening when somebody figures something out? We like to believe that knowing and thinking are states or activities highly characteristic of our species, and if so they are well worth systematic investigation.

Of course, we all acknowledge that other forms of life also "know" and "figure out" at some level. I am not thinking of a one-celled organism that recognizes a novel item as food and succeeds in ingesting it. That kind of knowledge and problem-solving is too far removed from the kinds of human knowledge and problem-solving activity that are so intriguing. A little closer is the kind of knowledge the household dog has about the spatial location of various familiar parts of the environment: the dog can either head for or avoid a particular object not present, in order to accomplish particular goals. As a member of a different species I find this example a familiar kind of knowledge. Presumably the dog can't talk to itself about the location of objects, but it has indeed come to know where they are.

I also recognize some dog behavior as problem solving of a familiar human type even though without all the attributes of the human activity. My own favorite demonstration was by a large, brown, standard-size poodle named Kittelbane who outwitted us many years ago. He liked to get out and roam around the neighborhood, causing a great deal of trouble, and we decided to build a fence around our back yard. With a considerable expenditure of money and time a six-foot high metal chain-link fence was built around three sides of the yard, the back of the house constituting the fourth side. I remember what a sense of satisfaction we had when the fence was completed and we opened the back door for the dog to run out into the yard. To our dismay he seemed to run immediately right through the fence and into the open neighborhood. Some careful investigation finally made it clear that there was about a six-inch space between a metal fence pole next to the house and the house wall, and the dog managed to squeeze himself through

that narrow opening and out into freedom. It was hard not to conclude that he had sized up the problem, had discovered a solution, and then was able to put it into immediate operation when the moment came. It may have been low-level reasoning but it was impressive, and it would be good to know just what went on in his central nervous system as he solved that problem.

Of course, human knowing and thinking are at a much higher level of complexity. Not only are the information to be known and the problems to be solved more complex, but our species has additional devices that may be used in these processes, such as the kind of inner speech that serves as an aid to thinking or a vehicle of thinking. To investigate how humans know and reason we could look at many different kinds of phenomena, and indeed cognitive scientists have done so. My own preference, as is natural from my educational and professional background, is to look at language. What does it mean to say that someone "knows" a language, i.e. can understand and speak a language (or for that matter read or write or sign a language)? How do people come to know a language they didn't previously know? How does people solve the problems of putting utterances together to say the things that they want to say, and how do they succeed in interpreting what someone else is saying?

One of the reasons for choosing language as the focus of cognitive research is that it is both highly complex and well described. Linguistic publications offer incredibly detailed accounts of certain aspects of language behavior, even though the authors may not regard description of behavior as their goal. This means that the experimentalist has a rich supply of well described bodies of knowledge to explore. A second reason for choosing language as the focus of cognitive research -- one that is reminiscent of Labov's 1964 criteria for selecting phonological variables for sociolinguistic research -- is the fact that most of the intricate patterning of language behavior is out of awareness, not easily accessible for verbalization by the language user. Naive speakers of English, for example, may be totally unaware of how they vary the length of vowels in relation to the voicing of a following consonant or the complex constraints they obey on forms of anaphora or the syntactic or discoursal conditions for their use of verb inversion. There is a further reason for the focus on language: the very terms of linguists' accounts -- the primes, rules, principles, representations, and the like that linguists put in their descriptions -- may be regarded as hypotheses about the actual processing of language by hearers and speakers. Investigators have, as it were, whole sets of ready-made hypotheses to be tested in whatever ways their ingenuity can devise. Of course, cognition-oriented research on language should not be seen merely as the testing of linguistic theory, but rather as an independent line of research that may indeed test linguistic theory or may stimulate new kinds of linguistic theorizing or may contribute to scientific understanding of our species that has little to do with linguistics.

But why <u>child</u> language? One deceptive attraction some of us have felt is the apparent simplicity of child language as opposed to adult language. Child language at any stage is presumed to have fewer words, fewer sounds,

fewer rules than the full language of the adult and hence might be analyzed more readily and once analyzed might give us valuable guidance for analyzing adult language. Alas this is not as valid as it has seemed. The route from zero to full competence is not a simple linear progression of successive approximations to an adult system. It was a great insight of Jakobson that child language at any stage is structured in the same senses that an adult language is structured although differing in detail, and this was an important step forward in the understanding of language development at the time, but it did not allow for the extensive individual differences in paths of development. These differences may come from differences in input, differences in cultural patterns of language socialization, or -- possibly of greatest interest to the cognitivist -- the varied choices of strategies and individual creativity on the part of the child.

No matter where one stands on the question of the relation between some kind of general cognitive ability and linguistic competence, the study of child language is a promising field. My own preference on this question, as you might expect, is for research that tries to discover relationships between specific non-linguistic cognitive skills and specific linguistic skills rather than global comparisons or claims of overall priority or autonomy. Fortunately I don't have to say much about cognitively-oriented research on child language, since -- as nearly as I can tell-- every paper on the program of this year's Research Forum is an example, I will limit myself to one small topic: the "acquisition of a rule".

In what is rightly referred to as a "classic paper" Berko [Gleason] in 1958 showd us that preschoolers could say <u>wugs</u> as the plural of a word <u>wug</u> that they had never heard before; that demonstration convinced a whole generation of psycholinguists that children acquired not just words and sentences but rules for constructing words and sentences. That article reflected a shift taking place from talking about "habits" to talking about "rules", a shift that has on the whole been valuable for increased understanding of language behavior in general and language development in particular. What I want to comment on tonight is my understanding of <u>wugs</u> 30 years later. Just four points,

1. Acquisition. If one can show that a child will extend a particular pattern to a novel item, this does not necessarily show that a child has acquired an adult rule. One must first show that the adults have such a rule and that the child has discovered the adult rule and puts it into operation in his/her own language production. Part of the problem is demonstrating the existence of a rule in the input languge. I have told elsewhere of my disillusionment in discovering that individual adults vary greatly in their formation of plurals of nouns ending in voiceless fricatives, and that the problems of finding the source of the children's patterns are severe (Ferguson 1979). The patterns of generalization to nonsense material may be quite different in adult and child, with corresponding problems in deciding on rule acquisition.

2. Habits. Psycholinguists, after some chastening research experiences,

have concluded that not all generalizations to new material represent rules in operation. In a recent article in Cognition Lachter & Bever assert, "The emergence of overgeneralization is not unambiguous evidence for the acquisition of a rule. Rather it may reflect the emergence of a statistically supported pattern of behavior" (239), and in the next sentence they unabashedly refer to such patterns as "habits".

3. Universals. Sometimes a pattern applied to new material has no source in the target language, but represents a universal tendency in language production. For example, when a child acquiring a language without final consonants attempts to reproduce final voiced consonants in nonsense stimuli, the child may devoice the final consonants even though he has a voicing contrast in initial consonants. Final consonant devoicing is a widespread phenomenon in the world's languages and in child language development.

4. Creativity. The child may create a rule which has some basis in the adult model but represents a wrong analysis. Indeed, some of the interesting details in the original [illegible] article are the incidental references to unique creations by particular children, noted at the time as amusing exceptions, but neglected in the subsequent research literature. They were eloquent testimony to the active, playful, system-building propensities of young learners, cognitive behavior more impressive in its own way than the acquiring of adult rules.

The moral of these points, if there is one, is that every advance in theory making or research methodology is likely to obscure some of the valid points of earlier theories or methods as well as providing clues to better ones, and my preference is always to look both backwards and forwards this way rather than to argue for the supreme correctness of a current method or theory.

Well, let me now turn briefly to the fourth kind of reason for studying child languge. I have tired your patience enough with the first three, and I'll spend only a minute or two on the last one. What I have in mind are interventionist reasons. Some people study child language because they are interested in education, remediation, or therapy. I hold the opinion that this kind of motivation is just as important as the other three, and I wish that more people interested in intervention would get involved in the study of normal language development. Some years ago when I first looked into textbooks on speech and language disorders I was surprised to find that very little space was given to normal development: more seemed to be known about deviations from normal than about the normal development they were presumably deviations from. Doubtless much can be learned from abnormal or atypical behavior that will help us to understand the more usual behavior, but the reverse is just as true, and it is good to see that educators and therapists are increasingly making use of biologically, anthropologically, and linguistically oriented research on normal development, in some cases contributing to it themselves. I have always been somewhat disappointed that we haven't given greater encouragement to the presentation of really

excellent intervention-oriented papers at our annual Research Forum, but I realize that there are other, larger forums for such papers.

In the preceding sections I have reviewed some biological, social, ognitive, and interventionist reasons for studying child language. I am not presenting these assorted reasons as a new approach to the study of child language. On the contrary, I am trying to recapture some of the awe-struck, interdisciplinary enthusiasm of the first modern students of child language before our various specializations narrowed our vision. I think, for example, of William Preyer, whose 1882 book Die Seele des Kindes (English title The mind of the child) exemplifies almost exactly the kinds of reasons I have discussed. I do not share Preyer's political views, and I feel his understanding of cultural differences was too biological for my taste, but he called for (and contributed) repeated observations of the same child, observations of different children, comparisons of animal and human behavior, anthropological comparisons (with children "in uncivilized nations") and comparison of normal and abnormal development. And all this was in an evolutionary framework and with a strong physiological bias. Unfortunately he accepted Haeckel's strict recapitulationism as a principle, but fortunately he ignored it when his own empirical observations suggested otherwise.

Let me close with three one-sentence quotations from Preyer that will show illustrate strengths and weaknesses (from Die Seele des Kindes as quoted in Eckardt et al. 1985:183):

"There is no generally valid and temporal sequence of sounds in the language of children, because each language possesses another sequence of sounds based on their frequency (in the respective language)."

Here Preyer pays no attention to the universal tendencies in human language phonology, pays some but not enough attention to the distinctive phonological organization of every language, and somewhat too much attention to the role of frequency. Jakobson and many child phonologists are the mirror image. They pay too much attention to phonological universals, some but not enough to the phonological organization of each language, and no attention at all to the role of frequency. We are now in a much better position than either Preyer or Jakobson to summarize the phenomena of phonological development (cf. Vihman 1988).

"Heredity must be without influence because each healthy child, who hears a language from birth, which was unknown to his ancestors, will still learn to speak this language perfectly."

Here Preyer gets it only half right. Very much like American structuralists of a generation or two ago, he realizes that there is no genetic disposition for particular languages, but he misses the possibility of a common nest-building plan for phonology. Finally,

"What is hereditary is the great plasticity of the entire speech

apparatus."

Here Preyer says just what I want him to say. He is one with those recent researchers who focus on human plasticity (cf. the stimulating book on human plasticity Lerner 1984, which, however, fails to deal with language!). It is by careful investigation of the exact nature of the playful, structure-building plasticity in child language development that we can begin to answer some of the biological, social, cognitive, and interventionist questions that are posed for us.

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